COMMENT ON "PHOTON SPLITTING IN STRONGLY MAGNETIZED OBJECTS REVISITED"

Stephen L. Adler Institute for Advanced Study, Princeton, NJ 08540

ABSTRACT

I point out that the results stated in the recent articles on photon splitting by Wunner, Sang, and Berg and by Wentzel, Berg, & Wunner directly contradict an earlier analytic and numerical calculation that I performed of the same process using Schwinger's proper time method, for strong magnetic fields and general energies below the pair production threshold. The results of Wunner et al. and Wentzel et al. do not show the expected low frequency behavior, nor have they been able to reproduce the expected small magnetic field behavior, suggesting that their calculations may not be gauge invariant.

In a recent letter in this journal, Wunner, Sang, & Berg (1995) have argued, on the basis of a detailed calculation of the photon splitting rate or absorption coefficient in an external magnetic field by Wentzel, Berg, & Wunner (1994), that the photon splitting process has a much larger rate than was previously believed. Their article suggests that the large splitting rate results from inclusion of effects associated with magnetic fields B of order $B_{cr} = 4.4 \times 10^{13} \text{G}$, and photon energies of order the electron mass m, which they state had not been done in earlier calculations. Wunner et al. correctly emphasize that their calculations, if correct, have important implications for cosmic γ and x-ray sources. I am writing this note to point out serious problems with the results of Wunner et al. which suggest that their numerical calculations may be in error (or may not be gauge invariant). I begin by noting that Wunner et al. have made significant misstatements of fact in their references to the earlier literature, when they state that "the astrophysical implications of magnetic photon splitting had to rely on simple analytical expressions derived by Adler (1971) and Papanyan & Ritus (1972) valid only in the weak-field limit $B \ll B_{cr}$...". This statement in fact applies only to the earlier letter by Adler, Bahcall, Callan, & Rosenbluth (1970) and not to the follow-up article of Adler (1971). In the Adler et al. letter, the authors showed that gauge invariance implies that the leading contribution to photon

splitting comes from the hexagon diagram; they then calculated the contribution from this diagram to the photon splitting rate, and discussed its physical implications. In the subsequent Annals of Physics article of Adler (1971), I applied Schwinger's manifestly gauge invariant proper time method to give a compact expression for the photon splitting matrix element, valid for arbitrarily large magnetic field and for any photon energy below the pair production threshold. (I used in this article the notation B for what I here term B.) The matrix element expression (for the allowed polarization case) is given on pages 610–611 of the Annals article, and a graph showing the results of a numerical evaluation is given on page 613; a sketch of how the proper time calculation is performed is given in Appendix I on pages 634-644 (the full algebraic details of the photon splitting matrix element calculation amount to over 100 pages, which I still retain in my files). An important consistency check on the proper time calculation is that it reduces, in the weak field limit, to the hexagon diagram result calculated in the letter of Adler et al. This was checked both analytically and numerically; in fact the graph of the numerical work plots the ratio of the exact to leading order photon splitting rates or absorption coefficients, which approaches unity in the small magnetic field limit. The numerical results show that for both $\omega = 0$ and $\omega = m$, the ratio of the exact absorption coefficient to the hexagon expression is monotonically decreasing as B increases from 0 to B_{cr} , and is only a weak function of ω , in direct contradiction to the results obtained by Wunner, Sang, and Berg.

On examining the article of Wunner, Sang, and Berg and the calculation of Mentzel, Berg, and Wunner on which it is based, I am struck by the fact that they never show, either analytically or numerically, that their photon splitting rate has the correct B^6 dependence for small B, nor do their numerical results show any evidence of the ω^5 dependence expected for small values of ω/m . Wunner et al. attribute their inability to reproduce the leading order results to an anomalously low transition from the leading order behavior, stating "Evidently at these field strengths the range of applicability of the weak-field, low-frequency form of the exact expression for photon splitting is restricted to much smaller photon energies than was previously thought". However, there is no precedent for such anomalous behavior in any of the extensive calculations which have been performed in quantum electrodynamics. I have always considered it axiomatic, in performing a complicated analytic and numerical calculation, that results must be assumed to be *wrong* unless one can reproduce one or more easily calculable limiting cases, and I find it disturbing that this criterion has not been applied by Wunner et al. I strongly suspect that the results obtained

by these authors are incorrect because they have not maintained gauge invariance. I note that they have calculated in a particular gauge (Landau gauge), rather than working in a general gauge and using gauge invariance as a check on the manipulations. This opens up the danger that any error or approximations which violate gauge invariance will introduce spurious contributions from terms of order B in the amplitude, whereas these terms cancel by gauge invariance and the masslessness of the photon, with the leading contribution to the photon splitting amplitude coming in order B^3 , with a coefficient proportional to the product $\omega\omega_1\omega_2$ of the incoming and outgoing photon frequencies.

Because of the potential astrophysical implications of the high photon splitting absorption rate claimed by Wunner, Sang, and Berg, it is important that their calculation and mine be rechecked by a third party, with the aim of understanding where the discrepancy arises and determining who is right. I will be happy to send a copy of the full details of my analytic calculation, and my computer program notes and listing, to anyone wishing to perform this recalculation, and I trust that Wunner et al. will be willing to do the same.

This note is based on a letter which I wrote to Drs. Wunner, Sang, and Berg in April, 1995, to which I received no response. I wish to thank John Bahcall and Bohdan Paczynski for urging that the issues be aired in a public forum. This work was supported in part by the Department of Energy under Grant #DE-FG02-90ER40542.

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